# MEMORANDUM

DATE: 5 Sept. 69

MEMO NO.: 6046

TO: M. A. Palmatier

FROM: D. Ellsberg

SUBJECT: Review of Emmett Keeler & Joe Newhouse's Draft RM-6108-RC,* "A Model of Medical Research Resource Allocation"

COPIES TO: C. Wolf, Jr., E. S. Quade, E. Keeler, J. P. Newhouse, V. D. Taylor, H. Porch, Dean Jamison

1. I am very dubious about issuing this paper as a Rand RM in anything like its present form. This analysis is not unique in using certain very restrictive assumptions; even so, the authors should more clearly indicate the restrictiveness of the assumptions and the sensitivity of their conclusions to these particular premises, preferably pointing out just how different assumptions might change the results. What seems far more serious is that the paper aims at an area of application where one would expect to find, in reality, the greatest departures from these particular assumptions. It is just in this area that the authors' neglect to be fully explicit about the limitations of their analysis is most misleading; on the other hand, the authors do stand to lose a great deal in the interest and plausibility of their findings if they do so, judging at least from the example of this reader.

2. A number of these assumptions show up in the second paragraph of the paper. The authors start by assuming utility to be a function of two variables: wealth, x, and "health status" which the authors take to be "reflected by the probability, P, of contracting the disease." There seems to be a simplifying assumption in the model here that the individual is susceptible only to one disease, so that P need not be interpreted as a vector. Moreover, it is assumed that the effect of contracting the disease can be denoted by a scalar: i.e., that one can, in effect, ignore both variation and uncertainty regarding all the various dimensions of a typical disease, including duration, intensity, discomforts of various sorts, varying symptoms, the probability of death, the likelihood of various amounts of income loss related to the illness, and the conditional likelihood of attracting other diseases. (The last consideration, very important in some cases, is ruled out of the model by the apparent restriction to one disease.) The authors next assume "for simplicity" that "a healthy individual's utility is proportional to x." This is a very strong assumption indeed, though the authors draw no attention to this. It is, in effect, a return to pre-Bernoullian assumptions in economics, and certainly pre-Von Neumann-Morgenstern utility theory. They claim this assumption is "not unreasonable, since expenditures on health research are small relative to total wealth." But local near-linearity is not the issue. Expenditure on health research is only one factor, and the least important one, in the calculation of the stakes. If the individual contracts the disease, even if he does not die, he faces the possibility of a diminution in his utility equivalent to the loss of virtually all his wealth: the linearity assumption can hardly be imagined to apply realistically over such a wide

*These comments were written hastily before departing on a trip and were made available in unproofread draft to the authors about August 22; they had been given orally to Newhouse earlier.

range of outcomes. When one includes the possibility of death, the range is even more extreme, and overall near-linearity still more unlikely.

3. The authors assume that "if the individual contracts the disease, we will assume that his utility is reduced to (1 - a)x. Where a ($\leq$ 1) is at a maximum, as presumably it is for death, the resulting utility is equivalent to the status of "zero wealth" i.e., to the loss of all the individual's wealth as measured in terms other than his health. Does an individual really reflect, in his risk-taking behavior a judgment that death is no worse than the loss of his wealth?

4. If "health status" is to be an argument in the utility function, it would most naturally be reflected as a state variable: having or not having "the disease." If, as in this paper, it is reflected by the "Probability (P), of contracting the disease," it seems ironic to make the comment, in justifying the assumption that utility is linear on P, that this assumption is reasonable "unless anxiety is important." But if P is an argument in the utility function -- as it probably should be -- surely it is precisely because it is both cause of and proxy for the variable of "anxiety." Is this not, after all, the basis for a great deal of expenditure on medical research: to relieve currently-healthy individuals of the burden of anxiety, a burden which they now bear either because they believe P to be high for them, or because the prospect of a particular disease or disfiguration (e.g., the loss of a breast) inspires so much apprehension? Surely anxiety should be represented in the utility function (i.e., even a simplified utility function for purposes of analyzing the allocation of medical research funds), and P is surely related to it. However -- as the authors suggest by their aside -- it is just in the presence of this emotional factor that the linearity of utility on P -- that is to say, the reasonableness and validity of the Von Neumann-Morgenstern utility axioms -- is most open to question.

Thus, take the example in footnote 3 on page 1. The assumption can equally be modified to read that an individual must be indifferent between a .1 probability of death, and the lottery whose prizes are, with equal probability, a .2 probability of death and a 0 probability of death. If 0 probability of death is regarded as either unrealistic (even in the short-term sense) or as not really appropriate to the area of application intended for this model, substitute for 0 an $\epsilon$, where $\epsilon$ is a figure so small as effectively to be regarded as negligible by the individual. Does the assumed indifference seem inevitable, where death figures among the outcomes?

Maurice Allais, in several well-known references, has critized the application of the Von Neumann-Morgenstern axioms in precisely this area of application: i.e., small or zero probabilities associated with extreme outcomes, either extremely good or extremely bad. Indeed, when I began to read this paper, it excited the thought in me that at last we had a very plausible example of a real life situation that should exemplify the Allais paradoxes, since expenditures on medical research,

like decisions on actual treatments, do involve affecting the probabilities of the most extreme outcome. It is precisely in this area that the most plausible case can be made that the Von Neumann-Morgenstern axioms will not apply. To say the least, it seems obligatory to say something about this possibility, and, I would say, to refer to the Allais critiques.

5. A perfect example of the Allais-type paradox appears in the footnote 1 to page 4, where the authors draw attention to the implication that "a person should pay more to reduce his chance of dying from 60% to 59% than from 2% to 0%." Quite so: this is precisely what is implied by the Von Neumann-Morgenstern axioms. Let the reader try it on himself. Next, try it on your secretary; and your wife. I will be very surprised if any of these express the choice required by the Von Neumann-Morgenstern axioms. As a matter of fact, I would still make this prediction if the example were changed to make the prescription from the Von Neumann-Morgenstern axioms even stronger. I will be surprised if the reader finds it easy to find a subject close at hand who would not prefer, e.g., to reduce his chance of dying from 1% to $\varepsilon$% (some very small, ideally "negligible", positive probability) than to reduce it from 60% to 58%.

This would be a typical example of an Allais "paradox"; but is it really paradoxical? Is it not, rather, the implication of the Von Neumann-Morgenstern axioms -- so calmly pointed out by the authors here -- that is paradoxical and indeed, close to fantastic? The pattern of preference contradictory to the axioms, if it does, as I suggest, appear generally, would probably depend upon the very aspect of "anxiety" and its alleviation referred to earlier. Perhaps it should be mentioned that the problem here does not depend upon the special linearity assumptions commented upon earlier, but upon the applicability in this context of the Von Neumann-Morgenstern axioms, which purport to be quite general.

6. Nothing is said of: a) the relevance to the utility function of when the disease might be contracted; b) the related question of the individual's rate of time discount, for the contraction of the disease; c) uncertainties concerning these factors and their bearing upon the individual's utility function; d) the interaction of a given disease with other diseases or weaknesses the individual might have, the impact of a given disease upon his vulnerability to other diseases and the probability of his contracting them within given time periods, and the effect of a given disease in general upon the aging process. In general, the model seems to assume that "the disease" is well-defined and certain in its effects, and moreover that interaction with the probability or incidence of other diseases can be ignored. These are not necessarily unacceptable assumptions, but they should be made explicit, and in some cases do deserve comment.

7. To assume, on page 2, that society's utility is "the average of individual utilities," in "a society of identical individuals"

presumably means that "society's utility" is the commonly-shared utility; otherwise, it would be meaningless to add or average these individual Von Neumann-Morgenstern utility functions.

8. The proposition on page 8 that "the signs of these derivatives accord with intuition" adds little positive weight to the plausibility of the model as "a reasonable approximation to reality." If the signs of the derivatives were "wrong" that might be a significant indication that the model was unacceptable, but the reverse is not true.

9. In summary: a) the restrictiveness and limitations of a number of the assumptions here at least deserve more comment; b) the fact deserves at least mention that by well-known arguments the realism and appropriateness of some of these assumptions are particularly questionable in the precise area of application intended here; c) although the paper could meet my objections by making some of these points explicit, it is my own reaction that it loses most of its interest when these limitations are recognized: at least, it seems hard to see how the results can be regarded as highly interesting unless the authors at least make an argument (which perhaps can be made plausible) that the conclusions they proposed are not highly sensitive to the restrictive assumptions they have made, or at least do not depend critically upon them.

Dan Ellsberg